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CROLL'S "CLIMATE AND TIME"*

Climate and Time in their Geological Relations; a theory of Secular Changes of the Earth's Climate. By James Croll, of H.M. Geological Survey of Scotland. (London: Daldy, Isbister, and Co., 1875.)

II.

MR. CROLL'S own theory about the distribution of heat by means of ocean currents is in intimate connection with his ideas as to the variation of climate in past time. His theory may be summarised as follows:—The Gulf Stream and other warm or cold currents are due entirely to the prevailing system of winds, which force the water along the surface, or even make it take a lower course; the return of the colder water from the Arctic regions being assisted by the difference of level caused by driving up the waters into a narrow basin, such as he supposes those regions to be. The result of this theory is, that if one hemisphere is colder than the other, the trades on that hemisphere will be strongest, and the resulting warm current will flow into the warmer hemisphere; any difference, therefore, in the mean temperature of one hemisphere from that of the other is augmented according to this theory by ocean circulation, whereas on Dr. Carpenter's theory the latter would have a counteracting influence. When, however, we take both theories into account, and also the effect of the distribution of land and sea, which is remarkably manifested by the two facts of the South Atlantic being coldest and the North Pacific also coldest, we see that we are not in a position to estimate the effect, if any of much consequence, of the different forms of ocean circulation on the occurrence of a glacial epoch, but must look for the causes of the latter independently.

Now what are the known facts to be explained? They are well described in various parts of this book, and the proofs of the less known are carefully given. We have first the widespread indications of a sheet of land ice in the northern hemisphere, reaching in some parts far down into temperate, if not into tropical regions; secondly, similar indications in the southern hemisphere during the same geological period, but without any proof of their being contemporaneous even in centuries with those in the northern; thirdly, a much milder climate than at present prevailing in very high latitudes in comparatively modern geological periods, though anterior to the glacial epoch; fourthly, that these changes from more intense cold to more intense heat have been going on throughout the whole of geological time; and lastly, that in the midst even of the glacial epoch, warm interglacial periods occurred. No satisfactory theory of the cause of the glacial epoch can leave any of these facts unaccounted for, still less contradicted. Sir Charles Lyell's theory, referring it to an alteration of the distribution of land and sea, does not well adapt itself to the magnitude of the phenomena indicated above in the first and second facts, and requires very sudden and violent changes to account for the fifth; and, moreover, it is shown by Mr. Croll that the distribution he indicates would have the very opposite effect to that supposed; geologists are therefore driven, however reluctantly, to consider the action of cosmical causes. Four theories founded on such causes have been proposed.

The first, that the solar system was passing through a cold region of space, may be dismissed at once; the second is that the sun is a variable star, and therefore the amount of heat received from him is variable; the third is, that the glacial epoch was due to a great obliquity of the ecliptic; and the fourth, Mr. Croll's, is that it depended on an increased eccentricity of the orbit combined with aphelion winters. We will discuss the last theory first, and examine Mr. Croll's proofs of it. In order to show how the eccentricity has varied in past time, and to find the periods at which it was a maximum or minimum, Mr. Croll has calculated by means of Leverrier's formula what its amount has been or will be, from 3,000,000 years past time to 1,000,000 years in the future, for intervals of 50,000 years, and has given a diagram and tables to illustrate the result. This must have been a most laborious task, but we are sorry to say that the results require confirmation. We have repeated the calculations for two of the most remarkable dates, near which the change is represented by Mr. Croll as very rapid from a maximum to a minimum, viz., 850,000 and 900,000 years ago respectively, and find that at the former date the eccentricity was ·0697 instead of ·0747, and at the latter date was ·0278 instead of ·0102 as expressed in the table. To satisfy ourselves that the mistakes are Mr. Croll's and not ours, we have recalculated also one of Mr. Stone's and one of M. Leverrier's results which have been used by Mr. Croll for the completion of his table, and in both instances have exactly verified them. The fact that the eccentricity was large when he represents it so, and small when he makes it small, seems to indicate that some approximating progress has been followed, and that possibly his diagram may give a *rough* idea of the changes of eccentricity for past time, provided of course that we agree to Leverrier's formula being used for such remote periods.

Assuming, however, that at some past date the eccentricity of the earth's orbit approached its maximum value, and that at the same time the winter of one hemisphere occurred in aphelion, what would be the result? In the first place the total annual heat received from the sun, which varies inversely as the minor axis of the earth's orbit, would be slightly increased, but not sufficiently to have much effect upon climate. The more important result would be that the hemisphere whose winter was in aphelion would have it very rigorous, and its summers would be very hot, while the other hemisphere would be enjoying a perpetual summer. It is on this that Mr. Croll relies for producing a glacial epoch, and we see that it involves the statement that the two hemispheres were *not* glaciated at the same time, while the other theories assume that they were.

Our question therefore is: Will an extreme difference between the winter and summer temperature produce a glacial epoch? The actual amount of heat received by either hemisphere may easily be shown to be the same, whether there are great or little differences between summer and winter, whether as to their length or their intensity, so that a glacial epoch could not be the *direct*

* Continued from p. 123.

result, and we must look to the indirect effects. While agreeing in the existence of many of those pointed out by Mr. Croll, we cannot think it quite so settled a matter as he does, as they do not all act in the same way. In the first place, though the total amounts of summer and winter heat together are equal in the two hemispheres, yet, since a larger proportion of the greater summer heat is *available* than of the smaller winter heat, the more unequal these are, it follows generally that *more* heat must be obtained, and therefore the more uniformly heated hemisphere will be coldest; but secondly, as Mr. Croll states, we must consider the formation of snow, *i.e.* take into account the latent heat of water and other physical properties. Some of his arguments on this point are rather circular, for whatever amount of heat is rendered latent in the melting of ice, as much will be supplied to radiation in the freezing; and no *increase* of ice would arise from this. There are, however, two points that seem to be made out. First, that snow and ice are better reflectors of light than almost any other substance, and therefore less heat enters into them; and, secondly, that moist air is much less transparent to heat than dry, so that the vapour raised by the sun in summer would be an *opposing* influence, whereas the frozen vapour in winter when fallen as snow would leave the air above freer for radiation. This result would overbalance that spoken of in the first place, and be a powerful influence in the production of a glacial epoch. The vapour, too, that was raised in summer would come in a large degree from the warmer tropics, and therefore continue to add each winter to the mass of the snow and ice in the more polar regions.

These seem to us to be among the most convincing of Mr. Croll's arguments, and they are in agreement, as he shows, with the condition of the earth at the present time as regards the more glaciated condition of the southern hemisphere, and they agree with what has been pointed out by Prof. Tyndall, that heat, to bring the snow in form of vapour, is just as necessary for a glacial epoch as cold to freeze it when brought. It has been argued by Mr. Murphy that under exactly the same circumstances it would be the more equally heated hemisphere that would be glaciated, as the cool summer would melt less snow; but according to the above theory the summer of the other hemisphere, though naturally hotter, would also be rendered cool at the earth's surface. We see that the whole of this argument depends on the relation of the atmosphere to heat rays, and what has been stated above has been experimentally verified; yet we are far from being fully informed on this point, and the example of the planet Mars, which is almost exactly under the circumstances of great eccentricity and winter aphelion supposed above, and yet has not much glaciation, teaches us that this may depend on other combinations of circumstances beyond those we have considered above.

The glaciation, Mr. Croll thinks, would be assisted by the deflection of ocean currents, on which he accordingly spends his strength; but the vertical circulation of Dr. Carpenter, no less proved than the influence of the Gulf Stream, would be antagonistic to this, and we may safely leave the unknown residuum out of consideration.

Such is Mr. Croll's theory of the cause of the glacial epoch, to the illustration of which he brings forward many interesting facts. Among these are the proofs he gives of the occurrence of warm interglacial periods. Some of these proofs are collected from other writers, but many are from his own observations, and consist of the intercalation of beds of fossiliferous sand between two masses of boulder clay, the fossils being often of a southern rather than of a northern type. He also refers to the records of borings collected by him and already published, which showed, in several instances, three, four, or even five boulder clays in succession, separated by stratified sands. These interglacial periods are certainly more easily accounted for on Mr. Croll's theory than on any other, as, owing to the numerous terms on which it depends, the eccentricity of the earth's orbit is liable to rapid changes. Many of the instances, however, of interstratified fossiliferous sands seem too insignificant to require so vast an apparatus as a cosmical cause to account for them; rather are they evidences of the dependence of temperature on the atmosphere, whose changes are much more comparable to those of limited beds. Another set of facts adduced by Mr. Croll in illustration of his theory is the evidences we have of glacial conditions in former geological periods, of which he gives a very useful summary, though it seems to us he goes too far in taking proofs of a *warm* climate to indicate glacial epochs preceding and succeeding it, on the ground that all warm periods *must* be interglacial—this is *lucus à non lucendo* truly. Indeed, the warmth of North Greenland in the Miocene period seems to us one of those facts which are not satisfactorily accounted for by the theory—for the eccentricity has seldom been much less than now—and our northern winters are in perihelion.

He thinks he can identify the glacial period proper, and those of the Eocene and Miocene periods, with portions of past time when the eccentricity has been great and yet rapidly changing to small; and attempts thus to get a measure of the length of a geological period, and hence with the aid of other theories and supposed measurements to arrive at the total length of past geological time. These speculations may be ingenious, but they can give no assistance to the solution of a problem of which we really have not yet the data. The title of the book leads us to believe that all the discussion about the glacial epoch is engaged in only to lead up to this, but we must regard that as a much more manageable and therefore interesting problem, and turn now to examine the other theories that have been broached to account for it.

The theory of the sun being a variable star is not in such an advanced state as to warrant a complete discussion from this point of view, and we have seen that mere absence of heat can never cover the land with snow and ice, and this theory therefore may be dismissed.

The only remaining one is that which accounts for it by increased obliquity of the ecliptic. This theory, which has recently been broached in different forms by Lieut.-Col. Drayson and Mr. Thomas Belt, has been espoused by Mr. Woodward in his address to the Geologists' Association, whose paper has been deemed worthy of insertion in the "Arctic Manual." Col. Drayson's form of it, which imagines that the whole mass of ice was formed every winter and melted every summer, may be dismissed as absurd. Not so Mr. Belt's. There can be no doubt that an

increase in the obliquity of the ecliptic would cause a greater difference in the seasons, and this difference we have seen to be the very basis of Mr. Croll's own theory; the results must be the same (and they are rightly seen by Mr. Belt), whatever may be the *cause* of the difference between summer and winter temperature. If this theory were the true one, it is plain that both hemispheres were glaciated at the same time, so that both theories cannot be true; but the matter of fact as to the synchronism or otherwise of the glaciation of two hemispheres can never in the nature of things be determined. But we have still left the question, Has there been or can there be any great change in the obliquity? Astronomers say no. Mr. Belt, however, thinks that the distribution of sea and land and similar causes *may* make it possible for greater changes to occur—a gratuitous supposition that Mr. Croll shows to be groundless. This cause, then, though it may have the general effect of lowering the temperature of temperate and Arctic regions, is not sufficient to cause a glacial epoch.

On the whole, then, there appear to be several independent cosmical causes which affect climate in a greater or less degree, and the probable truth is that a glacial epoch occurs when they all conspire to bring about the same result.

So far, by going from chapter to chapter, we have endeavoured to bring Mr. Croll's arguments into something like logical order. The remainder of the book scarcely admits of this; indeed, we think the author might well have bestowed more care in arranging his matter if it was intended to form a consecutive whole; as it stands, there is much that can only be called a miscellaneous collection of essays without any obvious connection. Among these are his accounts of observations on the North of England ice-sheet, and his speculations as to the direction of its motion. There are also two theoretical questions of great interest discussed—"The physical cause of the submergence and emergence of the land during the glacial epoch," and "The physical cause of the motion of glaciers." With regard to the first of these questions, there are undoubted proofs that great oscillations of the relative level of land and sea have taken place in recent geological times, and the question arises, Was it the land which sank and rose, or the sea which changed its level? It was rightly considered one of the grand discoveries of geology when it was first taught that the changeable sea was that which retained its constant level, and that the "eternal hills" had been but as yesterday beneath the waters; and this principle is not likely to pass away. By it all alterations of level have been ascribed to the motion of the land, and none to the rising of the sea. While agreeing, however, to the principle, we may doubt its universality, and may be prepared to entertain the question whether causes of limited extent may not operate to raise the level of the sea, and thus enable us to account more naturally for such rapid changes as are sometimes indicated. There can be no question but that any considerable amount of water which by the fact of freezing should be retained in either polar region, and form an ice-cap there, would correspondingly shift the earth's centre of gravity and draw the remaining water more over to the side on which the ice-cap lay; and the amount of elevation of sea-level might easily be calculated for any latitude, if we knew the extent of the cap and its manner of deposition, *i.e.* its shape; and the amount would be doubled if the ice-cap were first on one hemisphere and then transferred to the other. This calculation Mr. Croll attempts to make on the very ingenious method of approximation that supposes the ice-cap such as shall make the earth with the cap on one side a perfect sphere. The question can be worked out more directly, as has indeed been done, though with varying results, the mean of which indicates that the rise at one pole due to this cause would be about one-fifteenth of the thickness of the ice melted off the other. If, therefore, we want to account for an alteration of level of 500 feet in England, corresponding to about 600 feet at the pole, we should require to have somewhat less than two miles' thickness of ice on the antarctic regions now. While these figures represent data too far removed from the truth to be at all reliable, and there are, moreover, other causes that may affect the result, they serve to show the kind of thickness required—that it is not *twenty* miles, for instance. Are we prepared, then, to admit that there may be two or three miles of ice on the south pole? This does not appear to us at all an extravagant assumption, when icebergs have been met with 700 or 800 feet out of water, and which must therefore have been considerably more than a mile in total height. We do not think it therefore unreasonable to suppose that during the glacial epoch, or indeed at other times, when there was less ice at the south pole than now, the sea in our latitudes may have stood at a higher level, and that many of the elevated marine deposits and raised sea beaches are due to this cause, and not to depression of the land; for the latter we have no other evidence, and it would involve such vast changes in so recent times that we can scarcely believe would leave all the main valleys and hills as they were before the glacial epoch, and afford no evidences of post-glacial faults. This argument of course does not deny that there *have* been land oscillations during the period, but only that they are not the only ones.

This leads us to the last of the theoretical questions discussed by the author of this work—the physical cause of the motion of glaciers, the answer to which appears to depend upon what is the amount of the shearing force of ice. The remarks which Mr. Croll makes on the theory and experiments of Canon Moseley are very forcible. There is no doubt that the element of time enters largely into the amount of force required to shear ice, and that during this time heat is acting on the ice also, and consequently that satisfactory experiments can only be made on a glacier itself; and also that the theory of the dependence of glacier motion on *change* of temperature will not account for the greater descent in summer than in winter. But what is Mr. Croll's own theory? He, like Canon Moseley, calls in the agency of heat, and indeed, since heat obviously makes a difference in the amount of motion, we have only to find out *how* it makes this difference to determine the cause of the whole motion. He considers the motion of a glacier molecular, that the heat entering at one end melts the first molecule, which then descends by its weight and leaves room for the molecule above it to descend, when *it* melts. This may look very pretty at first sight, but the first molecule would never descend and *leave a vacuum behind it*; so the second

molecule must melt at the same instant, and so on to the other end of the glacier, which is absurd; and besides, what is there in this theory to distinguish a glacier from a common piece of ice? which on this principle ought to flatten out and not retain its shape as it does. Why also are we to suppose the molecule alternately to melt and crystallise when the heat is continuous? The mistake on which this explanation is founded seems to be the confounding of radiation with conduction. It is radiant heat that passes through ice, which is a very bad conductor. Ice at 32° F., heated by conduction, would certainly melt on the outside; the interior can only melt by the *absorption* of radiant heat. We cannot either understand the statement "that ice at 32° cannot take on energy from a heated body without melting," unless it is the exact equivalent of what we have just said; but then no heat could be transmitted, as it would be consumed in melting the ice, and if it were otherwise, still any amount of heat short of the latent heat of water might be "taken on" by a molecule without melting it.

We fear, then, that the complete account of the descent of a glacier is still a desideratum. The various theories may contain elements of truth, but none are entirely satisfactory.

As far as definite results are concerned, it will appear that Mr. Croll's book does not do all he hopes it may, yet we welcome heartily his attempts at reducing complex questions to arithmetical issues, for we thereby gain clearer ideas as to whereabouts the truth may lie, and certainly have the questions put before us in a more definite form. The vast problems with which he deals, and for the suggestion and discussion of which science is so largely indebted to him, are waiting for solution, and every attempt is valuable, both as showing us where to look and where *not* to look for help.

J. F. B.

SPRAGUE'S ELECTRICITY

Electricity; its Theory, Sources, and Applications. By John T. Sprague. (London: E. and F. N. Spon, 1875.)

THE author tells us in his preface that this book is "written chiefly for that large and increasing class of thinking people who find pleasure in the study of science, and seek to obtain a full and accurate scientific knowledge for its own sake, or as part of the necessary mental preparation for many of the departments of modern life." Our examination of the book itself would lead us to an opposite conclusion. We very much question whether any one of the class to whom the author refers will ever have the patience to read through this volume. Certainly they will have but sorry pleasure and anything but full and accurate information. The book abounds in foolish conceits advanced with a show of knowledge that cannot but repel every intelligent reader.

That we are justified in these strictures will be seen from one or two quotations. Here, for example, are some statements taken from chapter ii. in this book. At the outset the author asserts that the fundamental facts relating to frictional electricity given in "one of our standard electrical works (and it is just what all say) . . . are received as absolute truth by electricians . . . and yet there is scarcely a truth in them which is not overweighted by an error, and the simplest facts even are erroneously stated" (p. 17). Mr. Sprague, so far as we are aware, has never done anything to prove that he is able to sit in judgment on the intellectual giants among modern men of science. Mere off-hand condemnation of the laborious work of men like Sir W. Thomson and Prof. Clerk-Maxwell cannot for one moment be tolerated. Mr. Sprague seems to us to be like a child trying to turn one of the pryamids of Egypt upside down because he imagines it has been built the wrong way up. The best teaching is to let him try. This is how the author proceeds in his bold attempt. It is not true, he states, that bodies similarly electrified repel each other; "the repulsion is only apparent; the real cause of the motion is to be found in the attraction exerted by surrounding bodies" (p. 19). And with regard to the electrophorus, "that the dish forms the conductor from the dielectric to the earth, as all electrical books tell us, is an error which will come up for examination by and by" (p. 15).

According to Mr. Sprague the common explanation of induction is all wrong; "the real explanation is" given by him (p. 49). The rubber of an electric machine "is seldom made upon true principles" (p. 33); and as for the earth-connection to an electric machine, we are assured that it is merely imaginary; what we must do is to lead a chain to the floor or gas-pipe, and "hence the idea that we make a connection with the mass or surface of the earth" (p. 29). And further on (p. 40) we read—still concerning the machine—that "because both the poles are insulated and the circuits limited, we are freed from the *ignis fatuus* of the earth-connection." We presume the author does not mean the earth-connection is an *ignis fatuus*, but that the usual explanation is such; it is evidently so to him, for it has landed Mr. Sprague in a quagmire of crudities where we will not attempt to follow him. In these early chapters everything is attributed to "polarisation," a word which has for the author a consoling sound like that "blessed word Mesopotamia." We are told that it is for a similar cabalistic reason electricians employ the term "potential." Not understanding the term, and yet finding it necessary to say something about it, this is how the author discusses the subject: "The word [potential] is always used in place of tension or electro-motive force, because there is something full and smooth sounding about it; but the idea which really does belong to it is a pure mathematical abstraction which only highly trained minds can apprehend" (p. 154).

In another part of this book we meet with dark hints upon "Sprague's patent universal galvanometer," an instrument that is to "do for many purposes, without other instruments and without calculations, the work which at present requires the Wheatstone's bridge and expensive resistance coils, as well as many calculations." But, beyond exciting our curiosity, the author declines to go further, and so we cannot give our readers the benefit of this wonderful galvanometer, which combines "Pyscho" and "George Bidder" in one.

Notwithstanding the grave defects that quite spoil the early chapters in this book, it is only just to the author to point out that the latter part of the volume has considerable merit. Much useful practical information is to be